LETTERS-TO-THE-EDITOR

Regarding "Glomerular filtration rate after left renal vein division and reconstruction during infrarenal aortic aneurysm repair"

We read with great interest the article by Marrocco-Trischitta et al[1] and would like to offer some comments. This article is particularly important in the present climate of endovascular aneurysm repair (EVAR). In our unit, fenestrated EVAR devices are not currently popular and therefore juxta-renal aneurysms tend to be repaired by open surgery. Although the article indicates that only 1.3% of the patients undergoing open AAA repair required left renal vein (LRV) division, we believe that the need to divide the LRV during open abdominal aortic aneurysm (AAA) repair is likely to rise in the future. Hence, it is important to know whether reconstruction of the LRV is truly necessary.

The article concludes that reconstruction of the left renal vein (LRV) following its division during open repair of infrarenal abdominal aortic aneurysms (AAA) restores preoperative renal functional status without increasing the complication rate or total operative time. In the absence of a control group (ie, LRV divided during surgery but not reconstructed), it is erroneous to conclude that the restoration of renal function was due to LRV reconstruction. In our experience, LRV division has not lead to a profound deterioration of renal function postoperatively as shown in our paper published in 2000.[2] We accept that calculated glomerular filtration rate (GFR) may be a more sensitive marker of renal dysfunction than serum creatinine and are in the process of repeating our audit of LRV division during AAA surgery using this tool.

Only patients undergoing elective AAA repair were included in the study. The need to divide the LRV may be more crucial in emergency repair of AAA. In these unstable patients, LRV reconstruction may add to the operative time and may increase morbidity. Therefore, the results should not be extended to emergency patients undergoing AAA repair, and these patients need to be studied independently.

Our practice is to divide the LRV beyond (to the right of) the union of the left suprarenal, left gonadal, and left lumbar renal veins thereby maintaining some collateral circulation. We believe that this is sufficient to preserve the venous return from the left kidney and restore renal function over time. Another important factor in determining postoperative renal function is renal thrash (ie, microemboli) due to the juxta-renal position of the aortic clamp.

These and several other factors need to be addressed before LRV reconstruction can be recommended as a safe, effective, and necessary step in open repair of AAA. A well-designed randomized controlled trial in emergency and elective patients is the way forward.

Tapan Anil Mehta, MBBS, FRCS
James M. Clarke, MS FRCS

Department of Vascular Surgery
Norfolk and Norwich University Hospital
Norwich, United Kingdom

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doi:10.1016/j.jvs.2007.06.050

Reply

The interest of Dr Elsharawy and coworkers for our article is much appreciated. Indeed, we believe that our conclusions, rather than erroneous, were based on straightforward results. We showed that left renal vein (LRV) reconstruction after its division: (1) is feasible without significantly lengthening operative time; (2) is not associated with increased complication rates; (3) is not associated with renal derangements since glomerular filtration rate remained unchanged as in patients in whom the LRV was left intact; and (4) appears to be durable. We did not address nor draw conclusions regarding the risks of LRV ligation that others have previously reported. Hence, a control group of patients in whom the LRV was divided but not reconstructed was not necessary. We showed that LRV reconstruction is safe and viable and therefore should not be regarded as cumbersome.

We agree that our results were obtained in an elective setting and therefore can not be extended to emergent abdominal aortic aneurysm repair. Yet, the fact that LRV reconstruction may increase perioperative morbidity is to be demonstrated. One can argue that it may not be necessary in all cases, but the occurrence of renal venous hypertension seems unpredictable, and we find it unnecessary to take the chance.

In conclusion, our study showed that the reconstitution of LRV anatomic continuity is safe and re-establishes a physiologic condition. Advocates of LRV ligation have the burden of the evidence to prove their case.

Massimiliano M. Marrocco-Trischitta, MD
Germano Melissano, MD
Roberto Chiesa, MD

Vascular Surgery, San Raffaele Scientific Institute
Milan, Italy

doi:10.1016/j.jvs.2007.07.031

Regarding "Endovenous laser treatment of the short saphenous vein: Efficacy and complications"

Although Gibson and associates have addressed an infrequently discussed subject—the small saphenous vein (SSV; not the short saphenous vein)—the article has omitted some important details.[1] This article did not adequately incorporate the Venous Reporting Standards, current anatomic terminology, or a valid outcome assessment, such as the Venous Severity Score. Outdated references to the "short" saphenous vein or "Giacomini vein" should no longer be used.[2] What they call the "CEAP classification" is only the C portion. There is no information on E (etiology) or A (anatomy). For P (physiology), obstruction was excluded, so that I presume all limbs had reflux.[3]

The absence of Venous Severity Scores, particularly the Venous Clinical Severity Score, limits the assessment of this therapy's efficacy. These disease specific-outcome measures were developed to assess the utility of interventions in patients with chronic venous insufficiency,[4] while in this article we are left with only surrogate outcomes. Moreover, because the majority of patients had concomitant procedures to the great saphenous or perforating veins, the specific effect of SSV treatment is blurred.

Important details of diagnostic techniques and their criteria are absent, such as (1) the criteria for SSV reflux and the mean/median valve closure time, and (2) the criteria for perforator incompetence—reflux/diameter, or both. Because nearly 70% of the study population was class II and 136 limbs underwent perforator ligation, many of these perforators were in either class II or III. This is a relatively high

number of perforating veins to be found and treated in these "mild" classes. Were they really just branch veins?

The authors provide no information on where they accessed the SSV and how much was ablated, a point of debate in endovenous SSV treatment. The observation regarding the frequent relationship of the SSV to the intersaphenous vein, either as an extension or a direct continuation in 57% of the limbs, is a unique anatomic finding. I am curious, however, why no other incompetent veins of the popliteal fossa, such as the gastrocnemius or popliteal area veins, which are not accessible to their technique, were not detected in such a large series. These other veins of the popliteal fossa may, if incompetent, perpetuate reflux in the popliteal fossa, even if the SSV is treated properly. For example, Gillet et al[5] reported a 20% incidence of incompetent gastrocnemius veins in a series of 180 operations for SSV incompetence, a proportion similar to that described by Hobbs and Vandendriessche.[6]

The 5.7% incidence of deep venous thrombosis is troubling, particularly when compared with results with endovenous laser treatment of the great saphenous vein or open surgical series. Moreover, the incidence is actually higher in this series, when the patients with type C anatomy (no direct termination of the SSV in the popliteal vein) are eliminated. In these patients there is no chance of thermal energy from the laser tip directly damaging the popliteal vein. When the incidence of deep vein thrombosis is calculated on this basis, the incidence for at risk patients was actually 12%. This may be too high a price to pay for this less invasive approach to the SSV.

Thomas F. O'Donnell, Jr, MD

The Vein Center
Tufts-New England Medical Center
Boston, Mass

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doi:10.1016/j.jvs.2007.06.053

Reply

We thank Dr O'Donnell for his comments and agree that reporting standards and current anatomic terminology are important. The paper was submitted as "Endovenous Laser Treatment of the Small Saphenous Vein: Efficacy and Complications," but the anatomic terminology was changed by the JVS proofreaders in error to "short saphenous vein." A correction has been issued by JVS.

The purpose of our series was to show that EVLT of the small saphenous vein (SSV) was feasible and to determine the rate of complications. We defined *efficacy* as duplex-proven closure of the SSV and elimination of reflux. Dr O'Donnell alludes in his comments to the reason that we did not include the Venous Clinical Severity Score as a measure of efficacy. Because fully 78% of limbs underwent concomitant EVLT of the great saphenous vein, it would be impossible to tease out what the benefit of the SSV ablation was specifically in terms of any change in the Venous Clinical Severity Score. Instead, we defined efficacy very objectively, as stated above.

Dr O'Donnell is correct in noting that only the CEAP clinical classification is noted in the article. In the article and the accompanying table, we did call this "CEAP clinical classification" and not just "CEAP classification." Dr O'Donnell is also correct in assuming that all limbs had reflux.

Because the paper had to be edited to meet the length requirements of JVS, a number of procedural details and details regarding concomitant procedures (great saphenous vein EVLT, perforator ligation, and microphlebectomy) were not outlined. In our vascular laboratory, we define reflux by valve closure time greater than 2 seconds. Mean and median valve closure times were not collected in the data for the study. SSVs were treated only if they demonstrated reflux and were causing clinical symptoms or were cosmetically bothersome varicose veins. Criteria for perforator incompetence included both reflux and diameter. Perforator veins were treated only if deemed to be either clinically or cosmetically significant: ie, if they were tributaries into clusters of varicosities that either were symptomatic or bothered the patient from a cosmetic standpoint. Although other authors have found that routine perforator ligation is unnecessary in treating patients without deep venous insufficiency in terms of improvement in APG-measured hemodynamic parameters and clinical symptom score,[1] in our practice we have had suboptimal cosmetic results when we have left perforator veins to clusters of varicosities remote from the SSV or great saphenous vein untreated.

The length of SSV treated and the access point were left to the discretion of the treating surgeon (three surgeons were involved in the study). In general, the ablation was started 1.5 to 2.0 cm from the saphenopopliteal junction (SPJ) if no intersaphenous vein was present and just distal to the intersaphenous vein if it was present. Access was obtained in some cases as low as just above the lateral malleolus. We noted no difference in the incidence of paresthesias in patients with "low" access. Incompetent popliteal fossa veins other than the intersaphenous vein were not specifically tracked, but if present and causing clusters of varicosities untreated by EVLT, they were addressed by microphlebectomy of the branches.

We agree that a 5.7% incidence of deep venous thrombosis (DVT) is high (the incidence was 11.4% for type A anatomy, 2.9% for type B anatomy, and 0% for type C anatomy); however, we believe that our definition of DVT was very conservative. Any extension of clot into the SPJ was defined as a DVT. We would now describe these clots as endovenous heat-induced thrombosis (EHIT; as described by Kabnick et al[2]). We agree with Kabnick and colleagues that EHITs do not behave like de novo DVTs. Kabnick and associates' abstract suggested that clots flush with the saphenofemoral junction or SPJ do not need anticoagulation and recommended that clots extending into the saphenofemoral junction or SPJ filling less than 50% of the diameter of the deep vein be treated with low-molecular-weight heparin until the thrombus recedes out of the deep vein. Clots filling greater than 50% of the diameter of the deep vein or occlusive clots are treated with standard DVT treatments. According to these criteria, none of the limbs in our study with "DVT" would require standard DVT treatment, as all filled less than 50% of the popliteal lumen. Since completing this study, we treat our patients with EHIT with the above algorithm, and all have uniformly showed resolution of the EHIT within a matter of days without any bleeding or thrombotic complication. We believe that EVLT of the SSV offers patients a significantly easier recovery with reduced morbidity compared with stripping of the SSV. As EHITs have had a benign course in all of our patients both during and since our study and as the incidence of nerve injury is very low, EVLT of the SSV is the procedure of choice in patients with SSV incompetence in our practice.